



## The rehabilitation of deductive reasoning<sup>&</sup>

(*La rehabilitación del razonamiento deductivo*)

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**ABSTRACT:** The paper aims at the rehabilitation of deductive reasoning. As a paradigm of reliable reasoning, it should be applicable in every confirmation context. In particular, it should transmit inductive justification, so that if D justifies a hypothesis H, then D also justifies all deductive conclusions from H. Nevertheless, most current philosophers of science reject such a transmission principle as false. They argue against it by providing apparent counter-examples and also by showing that it is incompatible with common confirmation theories such as HD-confirmation and Bayesianism. I argue in the opposite direction that we should stick to the transmission principle and revise instead our justification theories towards more cautious justification procedures that respect the transmission principle. This will avoid further paradoxes of these theories and, in particular, will enable us to apply our confirmed hypotheses to new situations in a well-founded way.

**KEYWORDS:** confirmation, epistemic justification, transmission of justification, theory choice, Bayesianism.

**RESUMEN:** El artículo tiene como propósito rehabilitar el razonamiento deductivo. Como paradigma de razonamiento fiable, el razonamiento deductivo debería poder aplicarse en todo contexto de confirmación. En particular, debería transmitir la justificación inductiva, de modo que si D justifica una hipótesis H, entonces D también justifique todas las implicaciones deductivas de H. Sin embargo, la mayoría de los filósofos de la ciencia actuales rechazan este principio de transmisión, planteando contraejemplos y mostrando su incompatibilidad con teorías comunes de la confirmación, como la confirmación Hipotético-Deductiva y el bayesianismo. En contra de esta tendencia, argumento que deberíamos preservar el principio de transmisión, y revisar en cambio nuestras teorías de la justificación para trabajar con procedimientos de justificación más cautelosos que respeten el principio de transmisión. Haciendo esto evitaremos paradojas de las teorías ordinarias de la justificación y, en particular, estaremos en posición de aplicar nuestras hipótesis confirmadas a nuevas situaciones de manera fundamentada.

**PALABRAS CLAVE:** confirmación, justificación epistémica, transmisión de la justificación, elección de teorías, bayesianismo.

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## 1. Introduction

Deductive reasoning is a paradigm of good reasoning and should, therefore, be accepted in any form of reasoning and inference. Unfortunately, most contemporary philosophers assume that it is not generally applicable at a central point, namely in the transmission of inductive justification of our beliefs. This shows, in my opinion, that the existing conceptions of justification are inappropriate. We should modify them in such a way that they transmit justification and no longer produce transmission errors. This is a prerequisite for applying our well-founded beliefs and our scientific knowledge to new situations. I make a proposal how this modification of inductive justification might be done and show that it can solve further problems of inductive inference.

## 2. A Central Requirement for Confirmation Theories

The Transmission Principle (TP) claims that the deductive consequences of a justified belief are also justified. In particular, we want to assure that if evidence  $D$  (incrementally) confirms a theory  $X$  then  $D$  also confirms every deductive consequence  $Y$  of  $X$ . This is the TP I want to argue for.

**The Transmission Principle (TP):** From  $(D \gg_K X)$  and  $(X \Rightarrow Y)$  follows:  $D \gg_K Y$

That is to say: if  $D$  (for the subject  $S$ ) confirms the statement  $X$  (relative to his background knowledge  $K$ ) and  $Y$  is entailed (logically) by  $X$  (and  $S$  knows this connection), then  $D$  also confirms  $Y$  inductively (relative to  $K$  for  $S$ ). For simplicity, we always assume that  $D$ ,  $X$ , and  $Y$  are contingent statements, and, I think, the principle works for different explications of logical entailment represented by the double arrow and different forms of confirmation.

If the evidence  $D$  is strong enough to increase  $S$ 's degree of justification for  $X$  to an extent that she accepts  $X$ , and she knows that  $X$  entails  $Y$ , then TP requires that  $D$  also increases  $S$ 's degree of justification for  $Y$  to an extent that she also accepts  $Y$ . Of course, the TP can have further implications for the deductive closure of knowledge, depending on what one understands by justification in the concept of knowledge, but I will not pursue these additional questions here. In particular, I do not want to deal with complicated cases of "easy knowledge" in which we establish some of our meta-beliefs in a circular way (cf. Neta, 2013). My aim is only to specify a requirement for simple empirical confirmations in order to find the most adequate confirmation conception. Since incremental confirmation is the basis of absolute justification and acceptance of hypothesis, it seems to be a good idea to focus on this type of confirmation. Bayesians also have another concept of *absolute* confirmation, which is discussed in section 7.

Although we can often decide easily whether or not certain data  $D$  confirm a theory  $T$ , it remains difficult to develop a precise confirmation theory. The most popular candidate for such a theory among philosophers is currently Bayesian (incremental) confirmation, which holds that  $D$  incrementally confirms  $T$  iff  $P(T|D) > P(T)$ . However, if Bayesianism wants to explicate an objective concept of confirmation, it can eventually only reconstruct which logical or inferential relations have to exist between  $D$  and  $T$  (and our background

knowledge) for D to justify T (cf. Hawthorne, 2005). And therefore, we should first explore these basic confirmatory relationships.

To improve our theories of (inductive) confirmation, I would like to revive an idea from Hempel (1965) and argue for the transmission principle that Hempel has called the *special consequence condition* and regarded as a mandatory requirement for every confirmation conception. Unfortunately, many philosophers (as e.g. Wright 2003, Sober 2015) believe that the principle is wrong, because on the one hand, there seem to be transmission errors and, on the other hand, many confirmation theories are incompatible with the principle. This is, for instance, the case for the hypothetico-deductive confirmation account as well as for (incremental) Bayesianism. Hence, if we want to stick to the transmission principle, we have to modify these approaches, and, after all, move to more *cautious* forms of inference and confirmation. With a more cautious form of induction we can on the one hand comply with the transmission principle and on the other hand solve further problems like the tacking paradox.

The following short anecdote may illustrate a basic problem of inductive inference. An empiricist, a physicist, and a journalist travel to France and see just across the border three brown cows. The journalist says: "Look, in France the cows are brown." The physicist replies: "Be more cautious, we can only say that in France there are three brown cows." And the empiricist is even more cautious: "We can only say: in France, there are three objects that look from afar like cows and brown from one side."

Who of them is the best scientist? That depends on the particular situation and further background knowledge, and cannot be answered in general. The empiricist will never get deeper insights into the world with his very *cautious inference strategy*. He remains with the data but will not commit inductive errors. Thus, he advises us to use the *strict cautious strategy*: Do not go beyond what we can actually prove on the basis of our data. The journalist, on the other side, tries to draw much stronger conclusions about other situations with his *bold generalization strategy*. Both strategies pursue certain aims of science and we cannot tell easily how a good compromise should look like. These inference strategies correspond to certain conceptions of confirmation. We can ask which of the three conclusions is genuinely confirmed or justified by the observation. I propose, that the best confirmation theory should, at least, avoid the tacking paradox (or irrelevant conjunction problem) and subscribe to the transmission principle.

### 3. *Why we need the Transmission Principle*

First, I want to explain how I will use some of the basic concepts, such as "confirmation" and "justification", and explain some general epistemological assumptions that I will presuppose in the following. I will always use "confirmation" in the sense of *incremental* confirmation. Evidence E (incrementally) confirms hypothesis H if E supports H or tells in favor of hypothesis H or makes H more probable than it is without E. In many cases that is not enough for an *absolute* confirmation of H or a *justification* of a belief in H.

Nevertheless, (incremental) confirmation and (absolute) epistemic justification are closely related. In general, we are justified to hold a belief if the evidence confirming it is strong enough and there are no pieces of evidence disconfirming it or if they are negligible. Unfortunately, it is difficult to make these intuitions more precise in general since some pieces of evidence supporting a particular assumption may even be undermined by fur-

ther evidence and thereby lose their supporting function. For example, John tells me that  $p$ , which perhaps justifies my belief in  $p$ , but Susan tells me that John is a liar, undermining my evidence for  $p$ , and then James claims that Susan is the real liar, which perhaps rehabilitates my justification for  $p$  and so forth. And there are other complications that show that (absolute) justification is a more holistic concept and has to be based on our *total evidence* (Kelly, 2014). In the empirical sciences we can find cases in which  $E$  confirms and justifies  $H$  and we later invent a new hypothesis  $H^*$  that can explain  $E$  better than  $H$  and, therefore,  $E$  then speaks more in favor of  $H^*$  than of  $H$  and no longer supports  $H$  strong enough for the justification of  $H$ . But, justification or absolute confirmation is not my main topic here. I will concentrate instead on the simpler concept of incremental confirmation.

*Evidence* is determined functionally as “the kind of thing which can make a difference to what one is justified in believing or (what is often, but not always, taken to be the same thing) what it is reasonable for one to believe.” (Kelly, 2014). Sometimes I will also speak of data as confirming a hypothesis. In any case, I presuppose a moderate form of evidentialism, in which the justification to believe or accept an assumption  $A$  must in some way be based on our evidence and especially on the evidence that confirms  $A$ .

In many simple cases (without further disconfirming or undermining evidence) a (strongly) confirming evidence  $E$  will already be sufficient to justify a hypothesis  $H$ . In these cases the transmission principle has to guarantee that we are also justified to believe any deductive consequences of  $H$ . The counterexamples to the TP that we discuss in section 6 may serve as concrete examples in which the transmission of justification fails, and that seems to be a problem for our confirmation theory.<sup>1</sup> The bad effects of transmission failures of confirmations are often particularly visible in those cases in which an incremental confirmation alone provides an absolute confirmation or justification of a belief.

Our justified beliefs should guide our evaluation of further statements and, of course, our (rational) choices. But that couldn't work if confirmation and justification is not transmitted by deductive inferences. Only the TP allows us to apply our beliefs to new cases and usually we will not hesitate to rely on certain logical entailments of our well-founded beliefs. At least, it seems very natural that we can rely on a weaker claim  $W$  if we even can already rely on the stronger claim  $B$ .

Let's have a look at some simple examples. If my evidence provides some confirmation for the belief that there is a red table in front of me, then this evidence also provides at least the same degree of confirmation to the assumption that there is a table in front of me. Of course, few will want to deny that. Or imagine that we argue with the landlord about whether the contractually guaranteed minimum temperature of 20° C is reached in our apartment. He points out that the very reliable thermometer in the apartment points at 23° C ( $D$ ). That supports the claim ( $X$ ) that the temperature in the apartment is 23° C. Thus, he says, that he has fulfilled his part of the contract. But, now we argue that we regard  $D$  and  $X$  as well founded but not ( $Y$ ) that we have more than 20° C in the apartment, since that would follow only by applying TP to  $D$  and  $X$  and we reject TP. Therefore, we argue that he has to produce another justification for  $Y$ . He will hardly understand what we want him to do.

<sup>1</sup> I discuss the Bayesian distinction between incremental and absolute confirmation in section 7. For Bayesians the transmission of absolute confirmation is guaranteed due to the rules of probability.

In science we need the transmission of justification as well, at least if we want to apply our scientific knowledge. Certain Data D may increase our justification for theory (X) that an antibiotic A normally cures a disease K. With TP, there would be at least the same increase in our justification for the entailed claim (Y) that Joe, who has K, will probably be cured if he takes A. But, if TP fails, it will be very difficult to utilize our theory X. Deductive inference is usually the strongest form of inference we are equipped with. If it doesn't suffice to transmit justification and transport our knowledge, we cannot in general draw any justified consequences from our justified theories. Without TP, the physician cannot tell Joe that D supports believing Y. Therefore, Joe would have no reason to take antibiotic A.

This seems strange to me and I would interpret such a situation as indicating that our conception of justification as non-transmitting has an obvious deficit. Of course, inductive reasoning provides no truth guarantee. Even if A cures K in general it may not be successful in the special case of Joe, but at least D should provide some *prima facie* support for Y and thus for the recommendation that Joe should take A.

Can we, therefore, think of forms of confirmation and justification that are compatible with TP? In any case, we should give cautious confirmation a try; especially, since our existing theories of confirmation seem to have further deficits.

#### 4. The Co-Confirmation Problem and Cautious Induction

If we want to confirm and eventually accept an empirical theory T, we have to compare T's predictions with the data. To do this, we normally deduce an observable prediction D from T plus our background knowledge K and then check whether D indeed occurs. The best-known account of such a comparison can be found in the hypothetico-deductive conception of theory confirmation.

**HD-Confirmation:** From  $(T \wedge K \Rightarrow D)$  and  $(\text{not: } K \Rightarrow D)$  and D follows:  $D \gg_K T$ .

Unfortunately, the tacking paradox (or *co-confirmation problem* as I would like to call it) is a serious problem for this approach as well as for the underlying idea, and it is one reason for modifying the HD-account of confirmation. From the monotony of deduction, we find for any arbitrary statement A:

**Co-Confirmation:** From  $T \wedge K \Rightarrow D$  follows  $A \wedge T \wedge K \Rightarrow D$  and also  $\neg A \wedge T \wedge K \Rightarrow D$ . According to the HD-approach, this would mean:  $D \gg_K (T \wedge A)$  and also  $D \gg_K (T \wedge \neg A)$ .

In this situation A and non-A would be co-confirmed by D, even though D need not have anything to do with A. Thus, we have to improve the HD-approach at this point so that D justifies only T and not  $A \wedge T$  as well.

Furthermore, T itself may already be a composition  $T = H \wedge T^*$ , where H may be irrelevant for D, and we would again get a case of spurious co-confirmation with only  $T^*$  to be genuinely confirmed by D.

**Independence (Irrelevance) of H and D:** If  $H \wedge T^* \wedge K \Rightarrow D$  and also  $\neg H \wedge T^* \wedge K \Rightarrow D$  and  $(\text{not: } H \wedge K \Rightarrow D)$  and  $(\text{not: } \neg H \wedge K \Rightarrow D)$  then H and D are independent relative to K.

This form of irrelevance is a plausible indication that  $H$  is not genuinely confirmed by  $D$  in the HD-account. In order to make this explicit in our confirmation theory, some conceptions of relevance logic have been developed (e.g. Schurz, 1991; Gemes, 1993; Sprenger, 2010, 2013), which may help to solve the problem, but I am skeptical as to whether the problem can be solved completely at the syntactic level (see section 8). Thus, I prefer to discuss the intuitive underlying problem.

In any case, we need some restrictions in the definition of the HD-account. My informal proposal is that we always have to look for meaningful decompositions  $T = T1 \wedge T2$ , where  $D$  is irrelevant for  $T2$  (or even disconfirms  $T2$ ), so that in fact only  $T1$  is confirmed. Thus, if we find a decomposition in which  $T1$  and  $T2$  represent meaningful assertions and  $T2$  is not confirmed by  $D$ , then we should say that only a part of  $T$ , but not  $T$  as a whole, is confirmed by  $D$ . This will become obvious in the case of inference to the best explanation. Typically, those parts  $T1$  of  $T$  that are indispensable for an *explanation* of  $D$  are genuinely confirmed by  $D$ , whereas the conjunction  $T1 \wedge T2$  is only apparently confirmed. These ideas motivate my proposal for a cautious form of confirmation:

**Cautious confirmation:** If  $D$  confirms a theory  $T$  in the sense of some confirmation theory (such as the HD-confirmation or inference to the best explanation) and there is no meaningful decomposition of  $T$  into a conjunction  $T1 \wedge T2$  such that  $T2$  is not confirmed by  $D$  (relative to  $K$ ), then  $T$  is cautiously confirmed by  $D$  (relative to  $K$ ).

This eventually leads us to the general conjunction condition, according to which a conjunction  $A \wedge B$  is cautiously confirmed by  $D$  if and only if both  $A$  and  $B$  are confirmed by  $D$ :

**Conjunction condition:**  $D \gg_K A \wedge B$  iff ( $D \gg_K A$  and  $D \gg_K B$ ).

The conjunction condition (Hempel, 1965) can serve as a first guideline for the explication of a conception of cautious confirmation that will respect the transmission principle. My thesis is that genuine confirmation always has to be cautious.<sup>2</sup>

## 5. The Principle of Theory Strengthening

Further motivation to work with a cautious form of confirmation is to avoid becoming committed to the principle of theory strengthening (called Converse Consequence condition by Hempel), which is supported by most approaches to confirmation, and which is neither plausible nor compatible with the transmission principle. It says:

**Theory Strengthening (TS):** If  $D \gg_K X$  and  $Y \Rightarrow X$  then:  $D \gg_K Y$

<sup>2</sup> In order to avoid possible confusion one of the reviewers proposed always to speak of “genuine” instead of further introducing “cautious” confirmation, but I want to be more modest and only claim that cautiousness is a necessary condition for genuine confirmation. Nevertheless, genuine confirmation may have further aspects as well that I will not address here.

The principle TS seems to be far less plausible than TP, for why should D also support any stronger theory Y if we only know D to be sufficient to confirm the weaker theory X? In particular, it leads to the already described problem of co-confirmation of irrelevant conjunctions.

Nevertheless, TS is a typical consequence of our confirmation accounts. If X entails D then, of course, the stronger Y entails D too. Thus, from HD-confirmation follows TS, and, since Bayesianism respects these logical relations, at least for data that are entailed by X we find the same problem for incremental Bayesian confirmation. Bayesians therefore have to deny TP. For example, Pitts (2013, p. 2119) says: “Another possible source of confusion is the special consequence condition on confirmation, which Bayesians must reject”. Some Bayesians hope to soften the impact of the tacking paradox by proposing that the stronger theory Y is less strongly confirmed by D than the weaker theory X (Crupi & Tentori, 2010). But, that depends on the special choice of a Bayesian confirmation measure (with respect to the likelihood ratio measure X and Y may be confirmed equally strong by D) and can at most be seen as a first weakening of the co-confirmation problem.

One problem with TS is that it is obviously incompatible with TP, since from  $D \gg X$  and TS follows  $D \gg_k X \wedge T$  with an arbitrary theory T, and, therefore, we get with TP:  $D \gg_k T$ . Thus, D eventually confirms any theory T. This “anything-confirms-anything problem” is, of course, disastrous for any confirmation theory. However, as we have seen, the HD-confirmation and also incremental Bayesian confirmation are in their classical versions committed to TS, and even other approaches such as Likelihoodism or Classical Hypothesis Testing are affected in a similar way. In order to avoid the problem of confirming anything, we have to move on to more modest conceptions of confirmation that reject TS and fulfill the conjunction condition. These modifications are obviously necessary but not easy to implement. In addition, some philosophers argue against TP with difficult examples.

## 6. Counterexamples to the Transmission Principle

Unfortunately, several apparent counterexamples against the TP have been discovered. The twin example is often referred to as such a case of *transmission error* (cf. Wright, 2003; Moretti & Piazza, 2013). It is about two identically looking twins Joe and Jim:

(Twins) Our observation D that someone before us looks like Joe justifies the assumption (X) that Joe is standing in front of us. From X follows (not-Y) that Jim is not standing in front of us.

But, intuitively, D does not support not-Y because D speaks in the same way for the fact that Jim is standing in front of us as it does for the fact that it is Joe. This shows again that we should be more cautious in our inductive inferences. If we assume or discover that an identical twin of Joe exists, we should say that D only confirms “X or Y” (Joe or Jim is standing in front of us) and not the stronger assertion “(X or Y) and not-Y” = X, since we have no special clues for the second conjunct (not-Y). And with cautious induction we face no problem for TP.

The Zebra example is quite analogous. We see a four-legged, horse-like animal with black and white stripes before us (D). This justifies the assumption (X) that a zebra is

standing in front of us. From X we can infer (Y) that there is no mule with painted stripes in front of us. But this is not really substantiated by D if we know that the possibility of a perfectly painted mule with zebra optics is real. Then, we can cautiously justify only the corresponding disjunction: A zebra or a mule with painted stripes is in front of us (or more generally: a zebra-like-looking animal is standing in front of us). Inductive inference is, of course, always dependent on our further background knowledge, which tells us what other relevant possibilities we need to consider (cf. Bartelborth, 2004).

If our background knowledge is still incomplete or false and if we, at least, assume that no identically looking twin of Joe exists, then D seems to confirm the stronger statement X. This changes as soon as we seriously discuss whether D can also confirm that there is no such twin. Obviously, D cannot justify X as long as the possibility of an identical twin seems relevant. In this case, we have to formulate our inference more cautiously and have to consider all relevant alternatives that our background knowledge allows. For a genuine confirmation of X, we need evidence D that speaks clearly in favor of X in comparison with its relevant alternatives.

We can give a similar conjunctive analysis of Elliott Sober's (2015) example, in which he likewise confirms only part of a conjunction, and where the transmission error arises because he mistakenly assumes that he has confirmed the complete conjunction. If one can spot in a game of cards that a certain card is red, this information D supports the assertion (X) that it is the Ace of Hearts (according to Sober), from which it follows (Y) that it is an Ace, but Y itself seems not to be genuinely confirmed by D.

A simple probabilistic analysis can help to reveal the problem in this case: For a deck of 32 cards we find for instance:  $P(\text{Heart} \wedge \text{Ace} | \text{red card}) = P(\text{Heart} | \text{red card}) \times P(\text{Ace} | \text{red card}) = 1/2 \cdot 1/4 = 1/8$  and  $P(\text{Heart} \wedge \text{Ace} | \text{red card}) = 1/16$ . In this case the information "D = the card is red" increases the probability of "Heart" but not the probability of "Ace" and the probability of "Heart  $\wedge$  Ace" is only increased by the first effect. Thus, we have, at most, a *genuine* incremental confirmation of the claim that the card is Hearts, but not of the claim that it is an Ace of Hearts. Perhaps only the claim "Heart or Diamond" is really confirmed. This is the idea of cautious confirmation.

The conjunctive analysis amounts to the following: In each case a statement H is divided into two reasonably independent statements and we have confirmed just one of them, but not the other. Whether this is the case cannot always be determined by a logical-syntactic analysis alone, because the decomposability of H depends on which predicates and other expressive possibilities (including modal ones) and which additional background knowledge we have that specifies the set of relevant alternatives that we have to consider. Furthermore, it is often not easy to translate statements of natural language (e.g. conditionals) into a logical language. Rather, it is about whether, in fact, H can be represented without a loss of content by a conjunction "A and B" of two statements. Then we should say that H is genuinely confirmed if and only if A and B are both confirmed.

Achinstein (2003) has already given us some grounds to doubt that a probability increase alone can provide a good explication of the confirmation relationship. According to Achinstein certain explanatory relationships have to be added. This is another form of a more cautious confirmation, and I would like to add that only those parts of our hypothesis are genuinely confirmed that are *indispensable* for the required explanation.

A philosophical application of the transmission principle can be found in Moore's famous proof of the outside world. Moore (1939) deduces from (X) "Here is a hand" the



realist proposition (Y) “There is an outside world”. This, however, does not seem to be a compelling refutation of the external world skepticism to most philosophers. Let us first have a look at how Moore could justify his statement X itself. Modern empiricists would presumably refer to seemings (or sense data) like (D) “It seems to me as if I perceive my hand before me” to justify X. If Moore, furthermore, wants to conclude that D also justifies Y he seems to commit a transmission error, since D does not support Y and, for most philosophers, Moore, therefore, has no convincing argument against the skeptic.

The crucial question in Moore’s argumentation is whether D actually justifies X or perhaps only a weaker proposition that allows some skeptical hypotheses to be true and avoids the transmission error. In a debate with a skeptic, we have to regard skeptical scenarios as a relevant possibility and can only justify a disjunction as for example “Here is my hand or I am hallucinating that I see my hand”, from which we cannot draw realist conclusions. A more cautious form of inference gives us the presumably genuinely confirmed conclusion.

The tacking paradox and the given counterexamples to TP are typical cases of transmission failures. It seems obvious to me that they occur due to a violation of the conjunction condition. Furthermore, the conjunction condition seems to be a plausible requirement for confirmations, at least if we are aiming at the transmission principle, because in these cases we can see that only a certain identifiable part Y of our hypothesis X is confirmed, but not other parts of X. Thus, I believe we can easily agree to the conjunction condition that can avoid these failures. Does the conjunction condition guarantee the TP? At least, it does so if we accept a trivial application: If X logically entails Y we can represent X in principle by the conjunction  $X = Y \wedge X$  and, therefore, according to the conjunction condition Y is confirmed by evidence E if E confirms X.

## 7. Bayesian Absolute Confirmation to the Rescue?

Bayesians distinguish between *incremental* and *absolute* confirmation. We find both notions in many reviews on Bayesianism (e.g. Huber, 2007; Sprenger, 2016; Talbott, 2016). Incremental confirmation means an increase in probability and absolute confirmation requires a high probability exceeding a certain threshold value  $k > 0.5$ . Therefore, “D confirms H” is explicated either by (1)  $P(H|D) > P(H)$  or by (2)  $P(H|D) > k$ .

Absolute confirmation has the great advantage that it can avoid the tacking paradox and that it is transmitted by deductions. A reviewer, therefore, has proposed that we should take it as the main conception of Bayesian confirmation and moreover as an easy solution of the mentioned problems.

Those Bayesians who place *absolute* confirmation at the center of their theory of confirmation can take TP to be a necessary characteristic of genuine confirmation. However, Bayesians in general don’t want to dismiss incremental confirmation and in many applications Bayesians only refer to confirmation as probability increase. For example, this is the case with Shogenji (2017), who investigates under which conditions confirmation is transitive, i.e., in which cases even inductive confirmation transmits confirmation, that means that  $x \gg_k y \gg_k z$  implies  $x \gg_k z$ . For this purpose, he proposes special probabilistic requirements. In particular, we achieve transitivity if x supports or weakens z only indirectly through its impact on the mediating y. In a corresponding Bayesian Net there would be no direct path from x to z, on which x could directly support non-z.

This result shows a close connection to my considerations. The conjunction condition can ensure this for the special case that  $y = y' \wedge z$  (so that  $y$  logically entails  $z$ ), because then  $x$  particularly confirms  $z$  and certainly not non- $z$ . But the question of the transitivity of confirmation is more general and we cannot usually expect confirmation to be transitive. However, we have seen that we should demand that deductive conclusions always transmit confirmation.

In any case, for many Bayesians incremental confirmation is their main concept of confirmation (e.g. Bradley, 2015; Weisberg, 2011; Sprenger, 2016; Talbott, 2016). There are good reasons for this. First of all, it corresponds more closely to our intuitive concept of confirmation, which I have discussed in section 3, whereas absolute confirmation (or confirmation as firmness) does not fit well with our intuitive idea of confirmation. Therefore, Sprenger (2016) points out: “This last point brings us to a particularly unintuitive consequence of confirmation as firmness:  $E$  could confirm  $H$  even if it lowers the probability of  $H$  as long as  $p(H|E)$  is still large enough. But nobody would call an experiment where the results  $E$  are negatively statistically relevant to  $H$  a confirmation of  $H$ .” In this situation, it is not intuitive to say that  $D$  supports  $H$  or that  $D$  confirms  $H$ , but rather that  $D$  seems to speak against  $H$  and thus seems to disconfirm  $H$ .

Second, the incremental notion of confirmation corresponds to concepts of confirmation from qualitative approaches such as  $H$ - $D$ -confirmation. Outside of Bayesianism we have no clear criteria for an absolute confirmation. If we can deduce  $D$  from  $H$  and  $D$  occurs,  $H$  is confirmed to some extent by  $D$ , but we have no indication in the approach itself how strongly  $H$  is supported altogether by  $D$  and our further background knowledge.

Third, the (evidentialist) idea that Bayesian degrees of belief should be determined primarily by updating with (objective) evidence or data seems very plausible. This requires a conception of whether some data speak in favor of or against a hypothesis (i.e. raise or decrease the probability of the hypothesis), which is just what we want from a conception of (incremental) confirmation. So, at least one important explication of “ $D$  confirms  $H$ ” in Bayesian epistemology is *incremental* confirmation. Thus, Bayesians can’t completely do without incremental confirmation (and, in any case, we keep the transmission problem for all forms of qualitative confirmation).

Finally, even a combination of both criteria (that could fix our first problem with absolute confirmation) cannot help us. If we combine the conditions (1) and (2) for confirmation, the absolute confirmation would be transmitted by deductive conclusions but not the increase of probability and thus confirmation again would not be transmitted.

## 8. *Genuine inductive inference without transmission errors*

At least one hard question remains: Is the conception of a cautious confirmation too restrictive? Can there be any genuine inductive inference from our data if our confirmation theory has to comply with the TP, or can the data  $D$  only cautiously confirm those parts of any hypothesis  $H$  that are logically entailed by the data  $D$ ? I want to show that a cautious justification of scientific theories or generalizations is indeed possible. And, please keep in mind that we already know several other problems with our established confirmation accounts that motivate similar modifications. There are paradoxes like the “grue” or the raven paradox and some further problems mentioned above. These problems, in any case,

give us a good reason to rethink inductive inference. Let's start with a simple case of extrapolation.

If we examine three peaches and all of them have a stone, that supports the assumption that the next peach will once again have a stone, and it even confirms (incrementally) the general assumption that all peaches have a stone. For most types of fruit, we expect a certain degree of homogeneity with respect to stones, because this is an important factor for the reproductive mechanism of the corresponding plants. If we, however, consider Goodmanian generalizations with gruesome gerrymandered properties simple extrapolations can already lead to contradictory predictions. And even simpler examples can show that we should not extrapolate all patterns that can be found in our data.

We find a similar problem of lacking projectibility in inductive logic and in objective Bayesianism. Does the observation of an object  $b$  that is  $F$  increase the probability of another object  $a$  to be  $F$ ? For Carnap's (1950) original confirmation function  $c$  we find:  $c(Fa \mid Fb) = c(Fa \mid \text{tautology})$ . The function  $c$  did not exhibit a genuine *induction property*. Thus, it admitted no learning from experience and, therefore, Carnap developed the new function  $c^*$  using special considerations about the structure of state descriptions.

Jon Williamson (2008) has discussed the analogous problem in the context of his objective Bayesianism, using the example of the hypothesis that all ravens are black. We would like to conclude from observed black ravens that also other ravens are probably black, but his Maxent update procedure doesn't provide this connection:

To derive the problem it is assumed that initially there are no constraints, and that, once the ravens have been observed, there is a single constraint induced by the evidence. This overlooks important knowledge that is implicit in the language, namely that  $Ba_1, \dots, Ba_k$  are all related inasmuch as they are all applications of the same predicate. If this information is not taken into account then no connection between the observations can be made. (Williamson, 2008, p. 344)

However, the information that the instances are all instances of the same predicate is obviously too weak to vindicate the induction property.

If we construct unnatural predicates (or even grue-like predicates) we often get non-projectible generalizations as, e.g.,  $U$  = "All objects with the letter  $e$  in their most common English nomination are mentioned in the works of Conan Doyle."  $U$  may be true in many cases, but there is no substantial connection between the predicates (or properties) involved and, thus, we should not extrapolate to new cases if we have found some true instances of  $U$ . A cautionary inference should not go beyond the data in such cases. We can construe a predicate  $F$  for every arbitrary set  $S$  of objects as " $Fx$  iff  $x$  is Element of  $S$ ", however, we get thereby many unnatural "properties", and we only expect sparse and natural properties to yield projectible generalizations.

In the debate about grue-like predicates we can even learn that we should not only differentiate between projectible and non-projectible predicates, but have to select complete projectible generalizations (cf. Bradley 2015, chapters 7 & 8). Frank Jackson (1975) has given an example in which even the predicate grue can be used in projectible generalizations. Thus, we have to clarify and explain for entire generalizations which are genuinely projectible. This seems to be the central task in order to solve Goodman's new problem of induction.

Subjective Bayesians, of course, can easily incorporate the induction property into their degrees of belief functions, but that does not answer the question of whether and

in what cases an extrapolation is justified. Inductive support for new unobserved cases may be nothing more than a “feeling of support”, namely one in which the equation  $P(Fa \mid Fb) > P(Fa \mid \text{tautology})$  is correct, but this equation is maintained only as a subjective interpretation of the degrees of belief held by the epistemic subject, and we may have no objective clues to its validity. I am looking for a more objective grounding of the induction property.

Let's consider a simple extrapolation: We want to confirm the generalization  $H = \forall x (Fx \rightarrow Gx)$  by its instances. The generalization  $H$  can, e.g., be (H1) “when a piece of metal is heated, it expands” or (H2) “when someone has a certain type of pneumonia and receives an antibiotic  $A$  then he will recover within a few days.”

Usually, we will test  $H$  by examining objects of type  $F$  whether they are  $G$  too. The more  $F$ -objects we can find that are also  $G$ -objects the more strongly  $H$  should be confirmed. This fits the HD-conception of confirmation, because, if we have the background knowledge that  $a$  is  $F$ , then we can conclude from  $H$  that  $a$  must also be  $G$ . Unfortunately, we encounter a number of known difficulties even for this basic method of confirmation, such as the *paradox of the raven*, the *grue-paradox*, or simple instances that appear to be counterexamples to this idea of instance confirmation, such as the following: (H) “All humans are less than 2.40 meters”. If we now find several people who are 2.39 meters tall, this observation should confirm  $H$ . But, in fact, it seems to count against  $H$ , because these people let us reasonably assume that soon there may be people who are taller than 2.40 meters. Our background knowledge tells us that there is always a certain variability in size with humans. If, therefore, some people have already reached a height of 2.39 m, then it is very probable that someday an offspring will occur that is 2 cm larger and thus refutes  $H$ . However, these counterexamples are very special cases, and in general we think a *confirmation by positive instances* should at least be possible.

But, the strategy of caution seems to threaten all these extrapolations. Let's assume that there is a domain  $M$  of  $F$ -objects  $a_1, a_2, \dots$ , (possibly finite) and that we have made the observation  $D$  that the first observed  $n$  of the  $F$ -objects are  $G$ . Then we can split  $H$  in  $M$  into a simple conjunction:  $H_{1-n} = Ga_1 \wedge \dots \wedge Ga_n$  and  $H_{>n} = \forall (i > n) (Ga_i)$ . Our datum  $D$  coincides with  $H_{1-n}$  (within  $M$ ) and therefore confirms exactly this part of  $H$  but does not seem to confirm the further part  $H_{>n}$ . Popper has always expressed a corresponding suspicion. If this diagnosis is accurate, we would have lost any genuine induction and we are only talking about a *deductive partial confirmation* of the hypothesis or a form of *content cutting* as Earman (1992, p. 98) has called it:

... the real question for inductivism is the one emphasized by Nelson Goodman in *Fact, Fiction, and Forecast*, namely, when do already observed instances confirm a hypothesis merely by content cutting (i.e. by entailing part of the content of the hypothesis) and when do they genuinely confirm it in the sense of the predictions about unexamined instances?

For a genuine confirmation of  $H$  the  $F$ -objects must be similar with respect to some basic properties that decide whether they are also  $G$  or not. At least,  $F$  and  $G$  have to be connected in some way. One possible solution of Earman's problem may be that *laws of nature* are projectible, and, in addition, I think that many (lawlike) causal generalizations are projectible too. At least, for an inference to the best explanation we need generalizations that can *explain* their instances. In different conceptions of explanation this is spelled out in different ways, but in any case, not every generalization will be abductively confirmed by its

instances. In order to produce explanations, e.g., James Woodward (2003) requires invariant (causal) generalizations, Michael Strevens (2011) in his kairetic account of explanation relies on the entanglement of H, and in the Best System Approach of David Lewis only the best systematizing generalizations would be explanatory (cf. Lewis, 1994). I will call these explaining hypotheses *nomic patterns* or (lawlike) causal generalizations no matter how we will explicate them.

At the very least, these nomic patterns must be more than mere conjunctions of individual cases, because for simple conjunctions our conjunction condition requires all conjuncts to be confirmed individually, since otherwise the conjunction itself would not be confirmed genuinely. In order to extrapolate a generalization H, the Fs and Gs should have a special connection that we don't find in a material conditional like  $\forall x (Fx \rightarrow Gx)$ . As a first approximation, we can think of Woodward's invariant generalizations as nomic patterns.

In particular, a nomic pattern  $N = \forall x (Fx \Rightarrow Gx)$  should have explanatory force and describe a causal connection between Fs and Gs that supports counterfactual conditionals of the form: if some object or system *s* would have been F then it would have been G. Furthermore, N should have some stability and show some invariance. The modal aspect of N is a part of N's explanatory power. It shows why an object *s* that is F *has to be* G and gives us a causal explanation of Gs. This is the case in many circumstances (or possible worlds) in which no interfering factors occur. If N applies to many objects and situations it also has some unifying force and is not decomposable in two conjuncts that are separately confirmable, because both conjuncts are not nomic patterns and, thus, not cautiously and genuinely confirmed by our data. A genuine confirmation should demand two things: First, the data have to fit the hypothesis and, second, we are justified to assume that the hypothesis, if it is true, is a nomic pattern.

Some accounts of laws of nature can explain what is at stake. Dispositional essentialism, for example, holds that if N is a law then it describes a natural kind that is characterized by several powers or dispositions that normally bring G about if no antidotes or interfering factors were present. It is even part of the identity conditions for F-ness that F-objects or F-systems have a tendency to become G as long as no interfering factors prevent that. Thus, if we have a good reason for the assumption that N is lawlike and we have observed some instances of N then we also have a good reason to believe that N is valid for further non-observed objects. In any case, we have to discuss further the basic problem of induction: If we have observed that some objects in a group of objects (e.g. in the extension of F) are G we need some special grounds to think that further objects of the group are also G. The objects in the group must have some sort of common inclination to be G in order for this inference to be licensed.

Of course, the new question is how we can determine whether a generalization is lawlike, and thus projectible, or not. This is a difficult question, but science always looks for laws of nature and not for accidental generalizations. We don't want to know that some people (accidentally) recover from an infection after taking substance A, but we always want to know whether they recover in a systematic way and *because of* A, and we want to transport this knowledge to new cases. We, therefore, usually look for causal generalizations in science, and there are special methods (as, e.g., controlled experiments) to check for causal connections. Furthermore, we have a lot of background knowledge to decide if an observed regularity may be a law of nature or is presumably only pure coincidence. This is

a demanding challenge for all empirical sciences independently of our requirement of cautious confirmation. And it demonstrates again that confirmation is a holistic enterprise that is not reducible to a simple (syntactical) rule of induction.

If, in a concrete situation, we cannot decide whether our hypothesis  $H$  is lawlike, then according to cautious confirmation, we presumably cannot extrapolate our data to new cases, and the transmission principle cannot be applied. In sum, we face a dilemma: Either we stick to the existing theories of confirmation and lose the transmission principle, and thereby crucial ways to apply our justified beliefs, or we have to look for the more complicated theories of cautious confirmation that comply with the transmission principle, but then we lose the simple conception of confirmation (for example as a probability increase). To gain knowledge that can guide our lives we should choose the second option. Furthermore, we can thereby take first steps to solve other paradoxes.

### 9. *Hempel's Raven Paradox*

The material conditional  $H = \forall x (Fx \rightarrow Gx)$  with  $F =$  “is a raven” and  $G =$  “is black” is logically equivalent to its contraposition  $K = \forall x (\neg Gx \rightarrow \neg Fx)$ . If we then test  $K$  according to our method of instance confirmation, we have to examine of non-black objects whether they are non-ravens. This observation would confirm  $K$  according to many standard confirmation accounts. Or, in the case of metals that expand when heated, we have to look for objects that are not expanding. If they are no metal, or were not heated, they could also confirm the respective generalization  $K$ . Furthermore, these data would also justify  $H$ , since  $H$  and  $K$  are logically equivalent. But that seems absurd. Some philosophers, as Hempel himself, bite the bullet and try to explain that even white objects can confirm the raven hypothesis. Bayesians are content with the fact that, with suitable background knowledge, the non- $G$ s that are non- $F$ s provide us at least with a weaker confirmation of  $H$  than the  $F$ s that are  $G$ s.

A better first answer to the problem seems to me to examine the modal aspects of lawlike generalizations and cautionary forms of confirmation. If we, e.g., regard our hypothesis  $H$  as a modal conditional, like a counterfactual conditional, it is usually not equivalent to its contraposition. Thus, the first step in the derivation of the paradox is blocked. Additionally, in the framework of cautionary confirmation, the contraposition of  $H$  is not confirmed by its instances, because it obviously is no nomic pattern (and, of course, cannot explain the data), and the paradox disappears. It seems to be an artifact of the representation of  $H$  as a material conditional and the simple form of HD-confirmation or instance confirmation. If we confine confirmation to its cautious form with nomic patterns, we can avoid the paradox.

And, perhaps, there are some further generalizations than just laws of nature that we can extrapolate. In the cautionary confirmation account we search for stable nomic patterns in our world by means of their individual instances and only extrapolate observed patterns to unobserved situations if we can reasonably assume that the pattern is a (lawlike) causal pattern or is, at least, *backed up* by such a pattern. If we have observed that persons with yellow fingers often have developed lung cancer then we can infer that other persons with yellow fingers will also develop lung cancer, at least, if we acknowledge the causal pattern (the common cause in this case) in the background. Our smoking behavior causes on

the one side yellow fingers and on the other side lung cancer. Therefore, the pattern “yellow fingers leads to lung cancer” has a certain stability too and admits some extrapolations, although it is not itself a causal generalization. And the corresponding contrapositions seem to be no nomic patterns.

Of course, we are more interested in genuine causal connections since they allow us to intervene and prevent certain unpleasant events, but some derived patterns are within certain limits also projectible and apt to make predictions. We may eventually call all these generalizations *nomtic patterns* and it is of course a central question of our learning from experience which patterns are nomic and how we can detect them. This is a holistic enterprise that is well described by inference to the best explanation in the framework of a coherentist epistemology.

## 10. Conclusion

How should we understand “confirmation”? I propose that the transmission principle should be part of our confirmation theory, since otherwise our confirmation concept will be ineffective. In addition, I try to argue that there are genuine confirmation concepts that actually fulfill the transmission principle. However, we must significantly modify our previous confirmatory theories in the direction of a strategy of greater caution to ensure the transmissibility of justification. This, at the same time, allows us to solve other problems like the tacking and the raven paradox. However, the cautionary strategy should not go so far that no inductive inference is possible at all. Here we have to look for an intelligent compromise.

Therefore, I outlined which burdens of proof we have to carry or which background knowledge we must have to be able to reason inductively. I think we need certain causal or modal assumptions that the empiricist always wanted to avoid. Only lawlike generalizations are genuinely confirmable, and that complies with the aims of the empirical sciences that are concerned with the discovery of natural laws and not with the detection of accidental patterns.

Perhaps, we can find other ways to maintain the TP and still allow for a genuinely inductive reasoning. But it has to be shown how that can be done. In any case, the TP ensures that our well-confirmed scientific theories are also applicable. To develop a more precise conception of cautious confirmation is a task for further research.

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